

OCCASIONAL PAPERS ON EUGENICS
NUMBER THREE

THE TREND OF
NATIONAL
INTELLIGENCE

The Galton Lecture, 1946

BY

GODFREY THOMSON

D.C.L., Ph.D., D.Sc.

Bell Professor of Education, University of Edinburgh

With a Symposium in 1947 by

SIR ALEXANDER CARR-SAUNDERS

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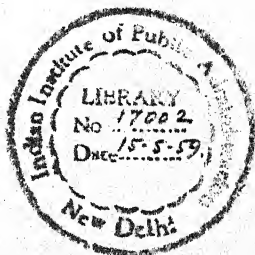
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THE TREND OF NATIONAL INTELLIGENCE¹

I HAVE during my lifetime had a great deal to do with the selection of children at 11 or 12 years of age for higher forms of education, and a good deal to do with the further selection which goes on at about 17 for entry to universities and colleges. I have, however, been very much worried by the fact that this process has an undesired and undesirable effect. It is intended, in all good faith, to be for the good both of the individual and the community, for it endeavours to educate each child in the way most likely to suit his ability and talents, and therefore most likely to make him happy, and this is also the way most likely to enable those talents to be of use to the community.

The Educational Sieve

But actually it has also another and regrettable result, especially in the case of the girls. The children chosen at 11 or 12 to enter on a longer and more difficult course of education are likely, on the average, to marry later (if at all) and to have fewer children (if any) than those who are not chosen. This is still more the case with those chosen later to enter colleges and universities. The men will marry later than they otherwise would have done, and a large proportion of the women will not marry at all. In short, the educational system of the country acts as a sieve to sift out the more intelligent and destroy their posterity. It is a selection which ensures that their like shall not endure.

It is clear that we nevertheless cannot do away with this selection of the most intelligent for the highest kind of education. It would indeed go on in some measure even were all our machinery of tests and examinations and interviews to be abolished. Somehow we must try to make social changes which will remove the influences causing more highly educated people to marry late and have few children. They are not greatly deterred from having children, I think, by any fear of having *themselves* to give up some material luxuries. They are somewhat deterred by the fear that a family of children will take away time and energy from their scholarly or scientific occupations. In the case of women they may indeed know that they will not as mothers

¹ The Galton Lecture delivered before the *Eugenics Society* on February 14th, 1946.

and wives be able or even allowed to continue these occupations. Most of all, however, they are deterred by the fear of not being able to educate a large number of children well.

My interest in the quantitative connection between intelligence and fertility dates from 1921, when I tested a large number of Northumberland children and noted facts which seemed to suggest such an association (1) (2). Since then, usually in conjunction with colleagues or students working under my guidance or in collaboration with me, I have made several experimental inquiries planned to elucidate this problem.

Intelligence and Size of Family

My general conviction is that there is a negative correlation between the "intelligence" of a child of about 11 years, and the size of the family of which he or she is a member, and I am fairly sure that the correlation coefficient is approximately -0.25 . Of its cause I am much less certain, but I think it is largely due to the later marriages of intelligent people, their restraint in producing fewer children, and the inheritance of their intelligence by their offspring.

Caution is necessary because it is very difficult to disentangle, in the estimate of a child's "intelligence", that part which is his inborn potential intelligence, and that due to his education, his home, his environmental chances. I do not myself think that environment and social inheritance explain more than a fraction, at most half, of the negative correlation actually observed, but it is difficult to test this. I shall describe below some attempts I have made to do so. First, however, let me expand my statement that (whatever may be its cause) there is a negative correlation of about -0.25 between intelligence as measured by an intelligence test and the size of the family to which the tested child belongs.

This is not a fact obvious to casual notice. Indeed, the man on the street will usually, in my experience, deny it and vigorously proclaim the many advantages, even the intellectual advantages, of belonging to a large family. An inverse correlation of -0.25 between size of family and intelligence leaves, however, plenty of room for cases where large families are intelligent and small families dull. It only implies that about 60 per cent. of the families are in agreement with the tendency, leaving about 40 per cent. of cases of discrepancy. No less than 20 per cent. of the families, in spite of the prevailing tendency, would be above the average both in size and in intelligence, and 20 per cent. would be below the average in both.

If we construct a grid or chequerboard table, showing along its one edge the size of family, and along the other edge the grade of

intelligence of the child who is tested, then although the column of the largest families will show many with low intelligence, it will show some with high intelligence, though not so many; and vice versa, some members of small families will be stupid, though more will be clever. There are therefore plenty of exceptions to the general tendency.

Casual observers, moreover, and even people like teachers, or journalists, or clergymen, do not see the whole population, but only a selected part of it. They know secondary school children, or slum children. Their acquaintances tend to belong to a class with large (or with small) families, tend to belong to a certain occupational or social stratum, and so on. That is to say, they are unacquainted with the whole of the data. Suppose we take such a part, where all the families are below average in size and above average in intelligence. That is the part of the data, for example, which will represent the relations, friends, and acquaintances of most of my hearers today. If we calculate the correlation between size of family and intelligence using only such a truncated grid, we find that it is, in this or any restricted sample of the population, so low as to escape casual notice entirely. Such selection always hides differences. But in a sample of the complete population, taken from slum and from suburb, from clerical and from manual occupations, from town and country, the negative correlation is unmistakable.

An Early Experiment

My earliest experimental approaches to our problem were indirect, and showed a negative correlation between the tested child's intelligence and the status of the father's occupation on a scale such as the Taussig (1). Since it was known that there existed a differential fertility among occupational groups, a negative correlation between the child's intelligence and the number of his sibs could be anticipated. In the years 1925-6 I planned a direct attack on the problem and was assisted by my student Dr. H. E. G. Sutherland. Our subjects were about 2,000 elementary school children (two nearly complete age-groups) in the Isle of Wight, about 400 boys of the Royal Grammar School, Newcastle-upon-Tyne, about 400 pupils of Moray House Demonstration School, Edinburgh, and 30 boys from Ryde Grammar School in the Isle of Wight. The size of family was ascertained by inquiry from the child tested. Only living children will, therefore, as a rule, be included.

Taking the elementary school children first, the percentage above average intelligence sank steadily, and with only one reversal, from 66 per cent. among the only children, to 39 per cent. among those belonging to a family of over seven. The percentages, as the size of

family increased, were 66, 64, 56, 54, 49, 41, 34, 39. The data from the grammar and the demonstration schools were not in discord with this general result, but, owing no doubt to being already selected samples, gave in themselves much lower correlations (3).

The chief blemishes in this research were (i) the use of group tests, which are more diluted by acquired verbal facility, especially reading ability, and (ii) the difficulty of the (possibly) unfinished families. This second difficulty, however, probably leads to an underestimation of the correlation. For if a child of 11 years is one of a small family it is probable that no more children will be born. But if he is a member of a large family it may be still further increased—though clearly much depends on his place in the family. Since the large families in this research are therefore more likely to be unfinished than are the small families, the negative correlations found are more likely to be underestimates than overestimates.

A Socially Homogeneous Group

We were chiefly concerned to test the possible explanation that the phenomenon of negative correlation is due to family circumstances and not to inheritance. Dr. Sutherland and I next therefore measured this correlation in a socially very homogeneous group, namely coal-miners working "at the face". The fathers of this group all belonged to the rank and file of coal hewers, everyone holding any kind of a distinguishing position, even of the lowest grade, being excluded. In 1926, 60,000 school children between the ages of 11 and 13, forming two age-groups attending elementary schools in the West Riding of Yorkshire, were given a group test of intelligence and were asked about their father's occupation and the number of their brothers and sisters. In 3,096 cases the father was a coal-miner. The correlations obtained were, in spite of the very homogeneous home conditions, still significantly negative (6).

Fatherless Children

It next occurred to me that I might obtain further insight into the problem if I took a group of fatherless children, in whose case the size of family was at least in part accidental, due to the father's death. I found in 1930 in Edinburgh 123 children born in 1916 or 1917 whose fathers had fallen in war before these children were a year old. Dr. Sutherland gave a group intelligence test to these, and also to a control group of 116 children with living parents, each child being matched by one of the same sex, same size of family, same age, and same school—as far as this proved possible. The correlation of intelli-

gence with size of family was in the fatherless group — 0.19, in the control group — 0.26 (8). The difference is, however, not statistically significant; the numbers were too few.

If the sole cause of the usual negative correlation were the foresight shown by intelligent parents, and if in our particular group of 123 children the size of family were entirely an accident, one would expect no negative correlation in this group. As it is, though the correlation is smaller (i.e. nearer zero) compared with the control group, the diminution is not statistically significant. This experiment therefore fails to give conclusive support to the explanation of heredity though it points in that direction. And we must remember that even in the case of these fatherless children the intelligence of the parents may have influenced the number and the spacing of the children up to the time of the father's death, and produced the negative correlation found.

At the same time we searched our Yorkshire data and collected 724 fatherless children (though not in these cases necessarily fatherless from birth) and a control group of 581 with living parents. The correlations between size of family and intelligence were practically the same as in the Edinburgh groups, i.e. the fatherless children showed a smaller, but not significantly smaller, negative correlation.

Shepherd Dawson's Data

After 1931 for several years I engaged in no further researches on this question, but then Dr. R. R. Rusk, of the Scottish Council for Research in Education, called my attention to data left by the late Dr. Shepherd Dawson and suggested that one of my students might work it over. This was done by Gerard S. A. O'Hanlon (13). Over 1,200 children between the ages of 5 and 8 years, whose parents were moving from a slum to a new housing area, had been given an individual Binet intelligence test. From these 1,239 cases Dawson had found a correlation of — 0.19. After some years they were retested, by which time the number still traceable was only 293 and it is these which form the subject of O'Hanlon's paper. Other information, in addition to the size of family, included room space, nutrition, income, mother's age at marriage, and years married when this child was born. The raw correlation between I.Q. and size of family (total number of births) was — 0.207. When, by the mathematical device of partial correlation, it was estimated what this correlation of — 0.207 would be in a group homogeneous in all the other factors mentioned above (room space, nutrition, etc.) the value found was — 0.293, that is, the correlation was even more pronounced.

It will be noted that in this research the test used was an individual

Binet test. It is therefore less open to the criticism that education, and not native intelligence, is being measured.

If we use these results, and similar results from other workers, as data from which to estimate whether the intellectual average of our race is sinking from generation to generation, an important point to remember is that in all the experiments quoted so far the families have been ascertained through a child of the family, and therefore childless marriages are omitted entirely. Nor are those potential parents who have not married included in the sample. It seems possible, and indeed somewhat probable, that these members of a generation—the childless—are, on the average, of rather high intelligence (consider, for example, the large band of unmarried women teachers). If the tendency is for intelligence to rise as we go from large families to those with few children, with two children, with one child, it seems very possible that it will continue to families with no children; that is to say, that the unborn children of the unmarried and the childless would have been, on the average, yet more intelligent (though we must remember that the childless group of adults, unlike the groups of parents ascertained through one of their offspring, will contain adults incapable of being parents). If this is so, then a negative correlation of -0.25 does not fully indicate the strength of the forces tending towards a deterioration of intelligence generation by generation. It is true that in a very important research Fraser-Roberts, R. M. Norman, and Ruth Griffiths found “only” children a little less intelligent than children with one sib, but they attribute this largely to the fact that illegitimate children were usually returned as only children (11). They felt sure that the decrease in average intelligence with size of family was really linear. They are further of the opinion that almost all sources of bias or of error, especially sampling error, would lead to estimates of the negative correlation that would be too low.

The Bath Experiment

Their article is in my opinion the best that has been written on this subject because of the completeness of the sampling and the excellence of the statistical work. My samples were in several cases fairly complete age-groups; but theirs can be said to be practically quite complete, all children (except a mere handful) whose homes were in Bath and whose birthdays were between September 1st, 1921, and August 31st, 1925, being ascertained, 3,401 in number, of whom 3,362 were actually tested, most of the missing 39 children having meanwhile left Bath and some having died. The Advanced Otis group test was used, but 1,271 of the children had also been given an indi-

vidual Binet test, so that a check was possible. The Binet I.Q. indeed "showed a significantly higher association with sib number than did Otis I.B.". There was thus "no suggestion that the verbal group test gave too high an estimate because of a possible social bias".

The statistical procedures adopted were excellent—they were discussed by the authors with Professor R. A. Fisher—and the conclusions are conservative. The correlation coefficient in which we are interested here was -0.224 (living children only included in the family size).

It is clear that if there is a negative correlation between size of family and intelligence, and if the conditions causing this are allowed to continue, then the average intelligence will sink generation by generation. There is indeed some direct evidence that this is so, for example, E. O. Lewis's investigation (7). Various calculations have been made based on correlations such as I have today discussed, calculations which do not ask whether the cause is to be sought in educational handicaps (which can be removed by legislation) or in genetic changes, but estimate the difference in intelligence quotient between a parental and a filial generation due to all causes. These calculations show an alarming decline of at least two points of Binet I.Q. per generation, and probably more.

The first such calculation, made by a method arithmetically somewhat similar to that about to be described, was, as far as I have been able to ascertain, published in the *Journal of Educational Psychology* for 1927, by Lentz, who divided each group of children by the number in the family, to obtain a distribution representing the parental generation (5). But the assumptions on which he apparently based his procedure were, it seems to me, erroneous, for they implied perfect correlation between parental and filial intelligence. In his book *The Fight for our National Intelligence* (1936), Raymond Cattell also uses the above method, and further estimates the distribution of intelligence in the next, as yet unborn, generation by *multiplying* each group of children by the number in the family, assuming in addition, it would seem, a perfect correlation between parental and filial fertility. However, both these assumptions of perfect correlation are unnecessary, and an arithmetical procedure identical with Cattell's second plan, but with other and much more reasonable assumptions, has today the support of Professor R. A. Fisher. I shall describe it in what I believe would be, in effect, his own words. It depends upon a comparison of the average I.Q. of *families* (counting each family once only) and the average I.Q. of all the *children* in these families. The former is an unbiased estimate of the average I.Q. of the parents, the latter an unbiased estimate of that of their children. The difference is the decline.

Indirect Calculation of the Decline

It is desirable, I think, to dwell for a while on this calculation, for it may seem at first sight to be performing the impossible, since it purports to estimate a decline in intelligence merely from tests administered to one generation. But first let me give an illustrative example, using the data from the Isle of Wight already referred to. There were in all 1,924 children tested, 840 in one year and 1,084 in another. The latter were a more nearly complete age-group and I shall

TABLE I
Average I.Q. of Parents and Children in 1,084 Families

(a) No. of Families	(b) No. in Family	(c) I.Q.	(d) Product <i>ac</i>	(e) No. of Children	(f) Product <i>ce</i>
115	1	106.2	12,213.0	115	12,213.0
212	2	105.4	22,344.8	424	44,689.6
185	3	102.3	18,925.5	555	56,776.5
152	4	101.5	15,428.0	608	61,712.0
127	5	99.6	12,649.2	635	63,246.0
103	6	96.5	9,939.5	618	59,637.0
88	7	93.8	8,254.4	616	57,780.8
102	(8)*	95.8	9,771.6	816	78,172.8
1,084			109,526.0	4,387	434,227.7
		Mean = 101.04		Mean = 98.98	

Estimated average I.Q. of the parents	101.04
Estimated average I.Q. of all their children	98.98
<i>Decline</i>	2.06

* Really "over 7". Using 8 will give a slightly underestimated figure for the decline.

confine the calculation to them. It is shown in Table I. Column (a) shows the number of children actually tested, and since only one year-group was concerned, this 1,084 is also the number of families. True, there may have been some twins among the 1,084, or some siblings born within a twelvemonth of one another, but such cases must be few. Column (b) gives the number of children in the family, including the tested child, and column (c) the average I.Q. of each group of children. In column (d) the average I.Q. of all these children is found to be 101.04.

This value is taken as the estimate of the average I.Q. of their

parents, on the very reasonable assumption that the average I.Q. of all parents will be the same as the average I.Q. of all their children *if they each have the same number of children*. Here each family is represented by one child, not specially selected, and therefore representative.

There is no assumption about the value of the correlation coefficient of parental intelligence with filial intelligence. This would have to be known if an estimate were required of the intelligence of the parents of *any specified size* of family, for in that case the phenomenon of regression, which is dependent upon the degree of correlation, would have to be taken into account. But when the average I.Q. of all the parents is estimated, regression plays no part. I have to thank Dr. D. N. Lawley for clearing up this point for me in discussions.

To return to our table: column (e) shows the total number of children of those parents, obtained by multiplying together columns (a) and (b). Then the average I.Q. of all these 4,387 children is obtained by multiplying the I.Q.'s of column (c) by the numbers in column (e), adding, and dividing by 4,387 as is shown in the last column. The assumption is again made here that the one child tested in a family is a fair sample of that family, and his I.Q. an unbiased estimate of the average I.Q. of the family. The child tested is in no way specially selected, and is equally likely to be above or below his brethren in intelligence.

The results of such calculations, of which a number have been reported, are remarkably in agreement, and with few exceptions give values for the decline in intelligence ranging from slightly below 2 points to well over 3 points per generation. The most widely known are those on the data of Dr. Raymond Cattell, gathered in Leicester (10), and on the data of Dr. Fraser-Roberts and his co-workers, gathered in Bath (11). These agree more closely than I at one time thought. The decline given by Cattell on page 42 of his book *The Fight for our National Intelligence* is 3.1 points of the units of his test; but I deduce from his diagram on page 269 of his article in the *British Journal of Psychology* for January 1936 that the standard deviation of these units was about 21, so that in Binet units his 3.1 would become about 2.3 points of decline. The Fraser-Roberts data give a decline in Otis units of 4.14 points, estimated by Fisher to be equivalent to 2.04 Binet units. These estimates make no allowance for the existence of individuals in the parental generation who were childless, and are therefore probably underestimates. Further, they do not seem to make any allowance for the fact—and I think it is a fact—that the number of years between generations is smaller for the class of parents who have large families, and larger for the parents of small families. If this is so, the values are again underestimates.

On the other hand, these estimates lump together all the possible causes which may have created the negative correlation between size of family and intelligence, and assume that these causes will continue to operate, whether they are social, and remediable by social and environmental change, or are genetic, and remediable only by selection. We, however, are very interested to know *whether* the causes are environmental or genetic. It was towards elucidating this that I planned my experiment with the children fatherless from birth.

The distinction between the two categories of cause is not sharp, for the adverse genetic selection which we fear to be going on is itself due to the social environment, at least in part. But a clear distinction can be drawn between an explanation which attributes the lower intelligence of large families to biological inheritance from their parents, and an explanation which asserts that their lower scores are directly due to the largeness of the family, due, for example, to the overcrowded home with no opportunity for study, due to the greater poverty when the wage has to support more children, due to the fewer books, the slummier district, the less well-staffed school in such a place.

The kind of social reform which the second class of explanation would call for may differ from the kind of social reform which would be needed to reverse the deterioration due to actual selection for poor mental inheritance. A flat rate of family allowance, for example, might make matters better environmentally, while further increasing the adverse selection.

Heredity or Environment?

Much therefore depends on the answer to the question how much of the scatter of intelligence in our population is due to inheritance, and how much to differences in schooling and education. About this there has long raged acute controversy, which has recently flared up again in America, after a period during which it was generally agreed that from 50 to 80 per cent. of the scatter was due to heredity. That fairly quiescent period followed the appearance of the *27th Yearbook of the (American) National Society for the Study of Education*, which was devoted to this problem and contained papers which, while conflicting, did so on the whole only within the above limits.

In 1940, however, there appeared the *39th Yearbook* of that society, among the articles in which there is much greater disagreement, between (one might say) a Californian school headed by Lewis Terman and the late Barbara Burks, claiming a very large influence for heredity, and an Iowa school, headed by George Stoddard and

Beth Wellman, denying this and making big claims for schooling. Among the kinds of research which are used in attempts to distinguish between nature and nurture are correlations between intelligence scores of identical twins, between foster children and foster parents, foster children and true parents, children brought up in a homogeneous environment like an institution, and so on. The Iowa school base their conclusions mainly on large increases in I.Q. following nursery school and infant school education, which are not confirmed by other investigators,¹ and are criticized as being either due to the inaccuracy and overhigh standard of baby tests, or to selection and statistical errors in the treatment of the resulting data. The differences of opinion are honest, and although I for my part think that the Iowa researches are unreliable, it is clear that we are far from being sure.

Of the 2 or 3 points of decline of I.Q. per generation which the differential birthrate data indicate, we are therefore unable to say with confidence how much is due to heredity and how much to environment. When, in a memorandum to the Royal Commission, I said that I feared the decline was one point, I meant one point due to heredity and to selection. It may well be more, but I hardly think it can be less. Particularly emphatic seems to me the fact that while the correlation between the intelligence test scores of siblings is about 0.5, that between all twins is about 0.7, and between twins after removal of all pairs where sex, bloodgroup, or other criterion indicates a double conception, this correlation rises to about 0.9. This last rise in particular seems inexplicable except by a genetic explanation, and suggests that a considerable part of what we call intelligence is inherited. Its genetic background is probably complex and in all probability a large number of genes are concerned.

That the mode of inheritance of intelligence is important in considering this matter has been illustrated by Professor J. B. S. Haldane by the analogy of the "ever-sporting" strain of stock. Since about Queen Elizabeth's time "double" stocks have been known, the doubleness, which is recessive, being due to a Mendelian gene (or perhaps a pair).

The doubleness is due to all the sexual organs of the flower becoming petals, and so double flowers are entirely sterile. The single flowers, in the ordinary strain, are either pure singles, which have only single offspring, or hybrid singles, which throw some doubles. In the ordinary strain, therefore, the proportion of doubles grows less and less as the generations follow one another—as we fear intelligence will grow less and less in human beings.

¹ I have since seen Miss Wellman's 1945 article, however, claiming agreement with her results in a large number of researches by others (14).

But in the "ever-sporting" strain, each generation is composed of approximately half singles and half doubles. Though the doubles are sterile, the singles of a generation always produce offspring who are in half the cases doubles. If mankind were like this, and intelligent people (double flowers) were quite sterile, still unintelligent people (single flowers) might produce a full quota of the intelligent, generation after generation.

I am afraid, however, that mankind is not like this. We are not divided sharply into intelligent and unintelligent, as stocks into doubles and singles. Intelligence ranges gradually, step by step, from genius, through the average man, down to the defective, and the distribution is approximately normal or gaussian, and is therefore probably due not to one but to many genes. The explanation of the ever-sporting phenomenon in stocks is that the "singleness" gene, in that strain, is lethal to pollen grains, and so all the surviving pollen bears the doubleness gene. Some human genes concerned with intelligence may possibly act like the gene causing singleness in stocks—which would slow down the drop in I.Q. per generation. But it is surely unlikely that many do, indeed I think it unlikely that *any* do.

However that may be, and whether the decline calculated from the differential birthrate be genetic or environmental, it is in either case a serious matter for the nation. If the lower intelligence of the members of large families is an educational result due to the overcrowding and poverty of the home, it could, with goodwill and suitable legislation, perhaps be cured in half a century. If it is genetic and inherited, a longer time will be needed to restore the loss, if indeed it can be restored, for although social conditions may alter so as to equalize the birthrate over the range of intelligence, it would be necessary to do more than this, it would be necessary to create a differential birthrate in the opposite direction, in order to recover what has been lost.

Need for a Direct Experiment

It is comparatively easy to obtain support for the kind of reform needed to equalize the cultural and educational handicaps of large versus small families. Some of these reforms might also assist in equalizing the differential birthrate, though others, however desirable for other reasons, might accentuate it. It is more difficult to obtain support for reforms tending to eugenic progress, partly because the man in the street thinks them impracticable, but largely because he does not believe in the alleged decline in intelligence. He is very sceptical about conclusions concerning the difference of intelligence between two generations when these conclusions are based entirely on

measurements made on only one generation. He demands a straightforward measurement of two succeeding generations, and I sympathize. *Actual measurement of two successive generations is desirable, indeed essential, and I would urge all who are in a position to facilitate such an experiment, or to contribute towards carrying it out, to do so.*

Even when measurements on two successive generations are made, however, there are still difficulties in the way of interpretation. The testees will probably be school children of about the age of 11, because that age, before children have scattered into different kinds of secondary school, is the time when a large random sample can most readily be obtained. The sampling would have to be equally complete in both generations, if the pedant will allow me to qualify the word "complete". If, for example, defectives in special schools were included in the one case but excluded in the other that would queer the comparison. Then there is the effect of migration. There may have been an influx of children of a new type or a different race or social class into the district or country. Further, the children of the later generation may have become accustomed to intelligence tests, and be thus enabled by familiarity with the situation to score higher marks than their fathers did when children. These and others are real difficulties. They should not, however, deter us from making the experiment with such precautions as we can devise. The question awaiting a decisive answer is of such importance that it would be worth while to go to much trouble, and to spend money freely, in order to settle it. I hope that this may be done in our time. If the decline is as rapid as Cattell fears, it should be detected (if the conditions can be equalized) in a period of even five years if a group of 50,000 children can be tested; and even if it is much less rapid, it should nevertheless be detected by an experiment on groups of 100,000 over fifteen years.

Factors of the Mind

I turn now to a different aspect of my subject. In the words of Karl Pearson, "Galton created the subject of correlation", and from correlation has arisen that branch of psychology in which I am most interested, and in which alone I can perhaps claim a certain measure of competence beyond the average—I mean the factorial analysis of human ability. I would like to devote a few minutes to explaining what it is, and how it may turn out to have a bearing on eugenics.

An intelligence quotient measures, more or less reliably, the powers of mind in a kind of linear fashion, as though all minds were alike except in the height of intelligence to which they can rise. But everyone knows that minds differ also in other ways, and that two

persons with the same intelligence quotient may have nevertheless very different kinds of mind—the one man may be a poet and the other an engineer. Mathematically speaking, one might say that the mind has not one dimension but many, and that the I.Q. is only a sort of *volume* of the mind which does not give any indication of its *shape*. These dimensions or directions of the mind are the “factors” spoken of in factorial analysis, which is based on a table of a large number of correlations between different mental activities.

Before we can have correlations we must have quantitative measures of the qualities to be correlated, and these are provided by the scores in different kinds of tests, some logical in nature, some with mechanical apparatus, some with words and some without, some using geometrical figures, some numbers, some drawings of right and left hands to be identified quickly, some based on codes and cyphers, and so on, an apparent medley of all kinds of tests.

When a number of persons have been submitted to say three or four dozen of these diverse tests, which *a priori* appear likely to call upon different powers of the mind, correlations between the tests can be calculated. There will be a large number of such correlations which can most conveniently be entered in a square table with the names of the tests written both along the top edge and down the side. Certain regularities then become apparent or can be discovered by suitable mathematical analysis.

In the first place, the correlations are mainly positive. Desirable qualities tend to be positively correlated in men. If we compare ability to supply the missing conjunctions in a piece of prose with the ability to say quickly whether the wheel in a mechanical model will turn this way or that when the lever is actuated, we find that these abilities apparently so different are positively correlated—perhaps not very highly correlated but still positively.

Clearly we can therefore attribute this tendency, if we like, to a general factor linking all mental activities, and British psychologists generally do so—the factor is Spearman’s *g*. But it is not necessary to do so, and American psychologists, following the lead of L. L. Thurstone, for the most part do not use a general factor but analyse the correlations immediately into group factors, as also the British psychologists must do with the residues of correlation left after the associating effect of the general factor has been removed.

The group factors are more or less the same to whatever school the factorial psychologist may belong. As the Americans at the moment have the lead in this work I shall in illustration mention some of the factors L. L. Thurstone and his co-workers claim to have isolated. The chief are a space factor, a perceptual factor, a number factor, and

two different verbal factors. British psychologists also recognize a verbal factor, though they have not split it into two, and a "space" factor, believed to be much the same as a "practical" factor found by others.

A Possible Compensation?

If it be true that there is going on a steady fall, generation by generation, in that intelligence which we measure or estimate by our present-day intelligence tests, may there not perhaps be compensation, it may be asked, in an increase in some other form of intelligence which may be more suited to the needs of the future. For example, is it possible that although verbal intelligence is decreasing, mechanical intelligence is increasing? There is against this hope the fact that nearly all, if not all, correlations between mental tests are positive. This seems to make the hypothesis improbable, but it does not make it impossible, for it is conceivable that this fact too is changing, and that the selection which is going on generation by generation, owing to the differential birthrate, may land mankind in an epoch when what we now call intelligence is actually correlated negatively with what will then be called intelligence.

I do not myself share this view. I think that intelligence is much more one thing than such a view would suggest. It is true that in the adult it takes different forms, due mainly, in my opinion, to his education and the influence of the environment in which he passed his formative years, though possibly also due to inherited powers which mature only in adult life. But although intelligence expresses itself in different forms, in its highest aspects it is always concerned with abstractions and concepts and relationships. Practical intelligence, as it is called, is of considerable importance in the world: but theoretical intelligence is of immeasurably greater importance. The clever garage mechanic may improve a motor-car engine. The student of thermodynamics or of atomic physics is much more likely to make the motor-car engine obsolete and replace it by a more efficient engine. And such men think in abstractions, often clothed no doubt in symbols of some sort or another, symbols which may be verbal, or mathematical, or, like Faraday's tubes of force, more mundane and materialistic, but symbols nevertheless, the real values with which these minds are operating being abstract relationships. I think there is a power of thinking abstractly which we can recognize in some of our fellow men whether they be chemists or classics, artists or mathematicians, craftsmen or administrators; and that though these men of high intelligence (as I shall call them) may, whether from education or heredity, possess different factors of the mind, they are alike in operating with them at

a high level. This level is what I call intelligence, and it is, I fear, being steadily lost to Europeans by the selecting power of the differential birthrate, those who possess it tending to have fewer children than those who don't.

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A NOTE ON THE CALCULATION OF THE DECLINE

IN the 1946 Galton Lecture on The Trend of National Intelligence I used purely as an illustration, on some data of my own, a form of calculation for estimating the average intelligence quotients of two successive generations, from the results of an intelligence test applied to a complete age-group of children aged 11. This form of calculation had been used by Raymond Cattell, and a closely allied calculation, to which I refer in more detail below, had been used much earlier by Lentz, and possibly by others. I understand that at any rate the kind of calculation I used in my lecture has the support of Professor R. A. Fisher.

Nevertheless correspondents have questioned the validity of the calculation, on the ground that it does not take account of the phenomenon of regression, which, they say (the correlation between parental and filial intelligence being about 0.5), would halve the rate of decline found. As these correspondents were thoughtful people, and as I myself formerly shared their view, this criticism may occur to others, and it is desirable that it be answered, which is the purpose of this note. The calculation I used is given on page 10. There were 1,084 children in the year-group. The number of only children was 115, of children with one sib was 212, and so on; and as with very few exceptions there is only one child per family in a year-group, 1,084 is both the total of *tested* children, and of families. The observed average I.Q. of each family-size is shown, and the average I.Q. of all the families is found to be 101.04. This is taken also to be the average I.Q. of the parents, on the assumption that this will be the same as the average I.Q. of all their families (but not, be it noted, of all their children).

If we further assume that each child tested is an unbiased specimen of his family, just as likely to be above as below his family average, we can estimate the average I.Q. of all these children, both the tested children and their sibs, by multiplying each I.Q. by the number in the family, as shown in the table. This average is 98.98, and the decline from the parents to all the children is therefore 2.06 points of I.Q.

The critics at this point protest that the parents in question are not an unbiased sample of their generation, for the parents of large families are too heavily represented. And this is quite correct. True, some doubt arises in one's mind as to the meaning of the words "their generation", since these parents are of different ages, but let that pass. The group of 1,084 parents (or midparents) contains, admittedly, more and more representatives of the larger and larger families.

But note that "all the children" are also a biased sample of the present generation, and biased in exactly the same way, for large families are represented exactly in proportion to their size, as were the parents. The comparison of these parents with all their children therefore seems quite legitimate, though admittedly neither the parents nor the children (all the children) form a fair sample of any actually existing generation. They are hypothetical generations, and this is felt to be unsatisfactory.

Let us therefore turn to the other form of calculation, used by

Size of Family	No. of Tested Children	Distribution of Parental Generation
1	115	115
2	212	106
3	185	61.67
4	152	38
5	127	25.4
6	103	17.17
7	88	12.57
8	102	12.75
	1,084	388.56

Lentz, which does compare two actually existing generations. The 1,084 children tested, being a complete year-group, represent adequately the present young generation, and their average I.Q. was actually 101.04, without any estimation. The 1,084 parents of the 1,084 tested children, however, contain too many parents of large families to represent *their* actual generation. But if we divide each group of children by their family size we obtain a distribution of 389 (388.56) parents which exactly corrects for this bias (except indeed that childless parents are ignored, a blemish which probably causes us to underestimate somewhat the decline).

Consider now a group of 389 children of these 389 parents who correctly represent the parental generation, one child to each parent (or midparent, shall we say, since in fact a child has two). These 389 children are distributed in exactly the same way, with regard to

family-size, as the parents, and the average I.Q. of each group of them is already known.

Family-size	One Child per Representative Parent	Average I.Q. of the Group	Product
1	115	106.2	12,213.0
2	106	105.4	11,172.4
3	61.67	102.3	6,308.5
4	38	101.5	3,857.0
5	25.4	99.6	2,529.84
6	17.17	96.5	1,656.58
7	12.57	93.8	1,179.2
8	12.75	95.8	1,221.45
	388.56		40,137.97

That the 115 children have an average I.Q. of 106.2 is an observed fact. They are the actual 115 "only" children whom we tested. The 106 children who come next are one half, a random half, of the 212 we tested, and there is no reason why their average I.Q. should differ from the average of 105.4 we found for the 212. And so on with the other groups. Forming the products, we thus find the average I.Q. of these 389 children to be $40,137.97 \div 388.56 = 103.30$.

Now comes the task of estimating the average I.Q. of their 389 parents, and we estimate it to be the same, namely 103.30. Note that the 389 children are spread over the whole range of intelligence. They are not an array of children all of the same intelligence, in which case regression would come in. They are the whole margin of a correlation table, not an array. So the estimate of the average I.Q. of the actual parental generation (ignoring, however, the childless) is 103.30. The *tested* 1,084 children are a fair sample of *their* generation, and average 101.04. The decline is 2.26 points, rather more than we found by the other form of calculation.

If we had used the idea of regression, however, we would have arrived at very nearly the same result, indeed in this particular example exactly the same result, if we take the mean I.Q. of the 389 children (103.30) as the centre towards which the regression tends, and any value whatever for the correlation of parental and filial intelligence. We cannot properly carry out this form of calculation with the data as here given, for the children of any one family-size are not an array of children with the same intelligence quotient, but are scattered widely in I.Q., the correlation between family-size and intelligence being only about -0.25 . If, however, we do treat them as though all the children of one family-size had the same I.Q., which is what I

think those who question the simpler process do, then the calculation, using 103.30 as the centre, and r equal to 0.5, is as follows:

Family-size	No. of Children	I.Q. Deviation from 103.3 halved	Product
1	115	104.75	12,046.25
2	106	104.35	11,061.10
3	61.67	102.80	6,339.33
4	38	102.40	3,891.20
5	25.4	101.45	2,576.83
6	17.17	99.90	1,714.95
7	12.57	98.55	1,238.91
8	12.75	99.55	1,269.26
	388.56		40,137.83

$$\frac{40,137.83}{388.56} = 103.30 \text{ as before.}$$

This last calculation, it is true, can be made to give almost any value for the average parental I.Q. by changing the centre towards which regression tends, but I think 103.30, the mean I.Q. of the 389 children, is the proper value to take.

The fact, by the way, that both these values, 101.04 for the children tested, and 103.30 for their parents' generation, are above 100 probably represents merely inadequate standardization of the test, or, in other words, that the population of the Isle of Wight scores higher on this test than the population of the areas where it was standardized.

In any case, the simpler argument is preferable to one complicated by regression. Here are 389 parents, admitted by the doubters to represent their whole generation adequately except for the omission of childless parents. The phenomenon we are investigating is a suspected decline in intelligence due to differential fertility. If we give each parent one child, there is no differential fertility, and in that case no decline due to this cause. The 389 children thus chosen have an average I.Q. of 103.30, and represent their parents' whole generation in this. Actually, however, some parents had more children; they had 1,084 in all, and these, who adequately represent the children's generation (being a complete year-group of it) have a lower I.Q. of 101.04. The decline from that generation to this is 2.26. *Whether it is due to biological or social inheritance is, of course, another matter, unanswered by these calculations.*

These arguments, of course, do not mean that regression does not take place. We are breeding from the less intelligent of the population,

and regression slows down the decline which this produces. The decline is not what it would be if parental-filial correlation were perfect. But it is the actual decline, the slowed-down decline, which is estimated by either of the above calculations, and the estimate needs no further reduction because of regression. Admittedly it is an indirect and precarious estimate, and a direct measurement of two complete age-groups of children, ten, twenty, or thirty years apart is very desirable. I for one would be relieved if such a direct measurement (the difficulties of which, however, are very great) showed no significant decline, and if a flaw could be found in the above argument.

DISCUSSION¹

SIR ALEXANDER CARR-SAUNDERS

TODAY'S meeting has been arranged by the Society in order to facilitate a discussion of Professor Godfrey Thomson's Galton Lecture on *The Trend of National Intelligence* which was delivered before the Society last February. A discussion of this lecture is bound to raise problems of much difficulty in relation both to the methods used in obtaining the data and to the interpretation of the data so obtained. Among those to follow me are Sir Cyril Burt and Dr. Fraser-Roberts, who, by reason of their own well-known work in this field, are so well qualified to discuss these difficulties. I propose to leave clear for them that aspect of the field about which they are so much better equipped to speak than I am, and to confine myself to a few general remarks suggested by the lecture.

We all know that eugenics, if not as old as the hills, is at least as old as the domestication of animals; for it was inevitable that those who found it possible to improve their herds by selective breeding should sooner or later conclude that similar methods should produce similar results in the human species. Though this conclusion was drawn, it was not made the basis of a programme until the second half of the nineteenth century; it remained as a prescription urged by ingenious minds for ideal societies. Then came the publication of Darwin's *The Origin of Species* which convinced men that the human race had evolved from lower forms of life. As soon as this view was accepted and it became evident that human evolution would probably continue in one direction or another, the suggestion followed that man could and should take the guidance of his own evolution into his own hands. It was in order to advocate this programme that Sir Francis Galton founded our Society.

In order to guide the trend of human evolution we must know which way it is now going. When the Society was founded nothing was understood about the mechanism of inheritance; it was possible to produce reasons for suspecting that there was differential fertility, and reasons for suspecting that social groups with varying reproduction rates were differently endowed. But it was largely guess-work. It was not merely permissible, but also in fact a public service, to

¹ A symposium on Professor Thomson's paper on January 21st, 1947.

speculate about these matters so long as the very slender basis of fact underlying them was emphasized. But since the foundation of the Society the mechanism of inheritance has been unravelled, and methods of measuring reproduction rates and such qualities as intelligence have been elaborated and applied. We have now passed out of the phase of guess-work. The change has come by steps, but Professor Thomson's lecture, which uses the evidence given by the new methods, marks the arrival of a new era in eugenics. That is the first point I want to make. We are now in a position to obtain and discuss evidence about the trend of human evolution.

The evidence which points to a decline in national intelligence rests on two series of observation. The first is that all social groups do not make equal contribution to future generations. There is no doubt at all that differential fertility exists, both between and within occupational groups. But we know much less about it than we should like to know. For instance, we do not know, and therefore cannot assess the influence of the differences in length of generations as between the different occupational classes; if we could, we should almost certainly find that the fertility differences between the groups are greater than now shown. We do not know whether the gaps between the groups in respect of fertility are widening or closing. We do not know what changes in this matter have taken place in recent years. The analysis of the Family Census will throw light on this matter. Speaking for myself, I should be surprised if the gaps between the occupational groups will be shown to have lessened perceptibly in late years; it seems to me that the social forces, which have produced these differences, still remain in operation and are likely to continue to do so for a considerable time to come. It seems to me even more likely that the forces which make for differences in fertility within groups will continue to be of importance. We cannot, in any case, say what will happen, but unfortunately we are not able in this case, for lack of evidence, to make informed guesses by projecting present trends into the future. We are left, in these circumstances, to our unaided judgment. There are those who hold that differential fertility is a passing phase in our society, that it began fairly recently and will disappear fairly soon. I do not share this view; I wish I could, because to any ill consequences arising from differential fertility only a limited period of influence could then be assigned. I am afraid, however, that if we accept the evidence as pointing to a decline in national intelligence we cannot console ourselves by the reflection that this decline will have a short duration and therefore a small effect.

The second observation, upon which Professor Thomson's conclusions rest, is that different social groups are differently endowed.

It is round this point that discussion is likely to centre. I wish to make only one observation. It is not uncommon to encounter the view that while the evidence demonstrates the existence of different average degrees of intelligence as between social groups which are in part genetic, the kind of intelligence in question is only one of several valuable mental qualities. The inference is then drawn that other valuable mental qualities may be unaffected by differential fertility. This inference assumes that valuable mental qualities are not correlated; but if I am correctly informed, the evidence is the other way and that they are positively associated. If this is so we cannot console ourselves by the thought that loss in one direction is compensated by gain in another. It seems odd that this idea of a supposed lack of association between valuable mental characteristics should have gained such wide currency; for it does not accord with personal experience. Those notable for musical, artistic, or literary gifts, for example, do in fact usually possess a degree of general intelligence obviously above the average.

I shall say no more about the argument contained in the lecture because I want to take the opportunity of replying to those who have said and doubtless will say again that, if the argument is true, we need not be much alarmed about the trend disclosed. To one school of thought I have already suggested a reply, namely to those who hold that the decline in one kind of desirable mental quality may be compensated by an increase in other desirable qualities. There are others who point to the large amount of misused or inadequately used intelligence in the population and draw the conclusion that the trend is of little or no consequence. It is true that there is a reservoir of insufficiently trained or utilized intelligence. But what is the relevance of this fact in the present context? Suppose that the coasts of an island were being rapidly eroded, and someone said to the inhabitants of the island: the loss of ground by erosion is of little consequence; there is still inadequately used land in the centre of the island. Would not this remark be rightly regarded as unhelpful? No one can say how much high intelligence we need in our society, but surely there is good reason to think that we could profitably use all that we have now got. If so, it follows that a decline in national intelligence is a national loss, irrespective of whether we use fully all the intelligence available. Quite another kind of reason was once advanced by a very eminent sociologist for taking a similar view. He said that it is not the silly sheep who bring troubles into the world but the highly intelligent Napoleons and Bismarcks. The only inference from this remark is a counsel of despair; let us not mind the disappearance of high intellect, because some who possess it misuse it. I think I need say no

more on this theme; human adventure on this planet springs from human intelligence. If it declined, the human story would become drab; there would be little worth recording, and in time no one capable of recording the dismal annals of a race deprived of the capacity for intellectual enterprise.

The evidence now available for assessing the trend of national intelligence is defective in many ways; Professor Thomson has pointed out, for example, that the evidence is all indirect. And there are many aspects of the matter which need the most careful scrutiny. We are only at the beginning of an exploration of a most difficult field of inquiry. It is therefore most urgent that the problem should be further investigated; for such evidence as we have tends to show a most serious state of things, and if the conclusions drawn by Professor Thomson are confirmed, very careful thought should be given to an attempt to find a remedy. The tools necessary for an investigation have been forged; they need sharpening, it is true, but even now they can be used with much profit. For the first time in the history of this country the possibilities of research in this field are promising. The universities are now equipping themselves with psychologists, geneticists, statisticians, and sociologists; they might well devote part of their energies to the elucidation of the subject of today's discussion. It is greatly to be hoped that they will do so. There is a special reason for drawing them in; in the investigation of a problem which is liable to arouse emotion and prejudice it is valuable to have contributions from as many independent centres of research as possible. It is the task of the Society to arouse and spread interest in this problem so that our greatly increased national resources for social investigation may be employed in its elucidation.

SIR CYRIL BURT

THE RELATION OF INTELLIGENCE TO FERTILITY

VALUE OF OBJECTIVE METHODS: (a) TESTS; (b) STATISTICAL ANALYSIS

THE remarkable feature about the discussion is the close agreement of all the contributors in regard to all the main points investigated. This agreement demonstrates the value of applying "scientific" procedures to social problems—i.e. of substituting empirical investigations by means of objective techniques for mere armchair debate.

However, in the present state of our knowledge, the answer given to the main issue still rests chiefly on a deductive inference. Here I am myself so completely in agreement with the conclusions reached by Professor Thomson in his Galton Lecture that I can do little more than echo or amplify his main points.

A. DEDUCTIVE (INDIRECT) EVIDENCE

1. *Tested Intelligence Negatively Correlated with Fertility.* The first premise in the argument is that there is a negative correlation between tested intelligence and fertility. Thomson assesses the correlation as approximately -0.25 .

My own evidence was chiefly secured during surveys in the London Schools. In my last report referring to the problem (*The Backward Child*, 1937, p. 102) I found that "the correlation between the intelligence of a child and the size of the family to which he belongs is -0.19 ". (This figure related solely to children attending elementary schools; if an allowance for pupils at other schools is included, the figure would be slightly larger.) Earlier inquiries on a smaller scale in London and elsewhere indicated figures of much the same order. Hence the figure is fairly constant for different times and different places. Fraser-Roberts's figure (-0.224 , obtained in the most reliable of all surveys) agrees.

2. *Tested Intelligence is Largely Innate Intelligence.* Tests of Intelligence, such as those devised by Binet, were designed to measure what Galton called "natural ability"; but they only do this imperfectly. Most psychologists now agree (a) that there is a "general factor" common to all cognitive activities (and it is encouraging to find that Thomson, who has been one of the severest critics of the arguments for this, now accepts this proposition); (b) that this "general factor" is largely innate and inherited. (Most psychologists,

I think, would agree with Thomson that the recent researches of the Iowa school are too unreliable to alter previous views.)

I agree that *at least* 50 per cent. of the variance shown by earlier intelligence tests (uncorrected) is due to heredity. Later methods (based on improved tests, checked and corrected in practical work by an individual study of each child) would give a far higher proportion.

3. *The Amount of the Calculated Decline.* The conclusion deducible from these two premises is that the average I.Q. is declining by about 2 points per generation, and that *innate* intelligence is declining by at least 1 point. (Thomson's investigations of fatherless children seem of special value as helping to separate innate and environmental influences.)

(a) *Methods of Calculation.* There has been some doubt as to how precisely the difference should be computed. Thomson bases his figure on a comparison between "an unbiased estimate of the average I.Q. of the parents" and "an unbiased estimate of that of their children": it seems conceivable that, if the comparison is stated in this way, a critic might doubt the possibility of getting a good measure of the I.Q. of the parents without testing those parents.¹

My own method of comparison was slightly different. Taking the actual number of children in each family, I calculated the average intelligence for the child population as measured. I then calculated the average we should expect if all the families had been of the same size. Here what we compare is not the average intelligence of the *children's* generation with the average intelligence of their *parents'* generation, but the average intelligence of the children's generation as it *actually* is with the average intelligence we should have *expected* with equal numbers in the family.

(b) *Results.* (i) *Average Decline.* From the figures I had obtained, I calculated that there would be a drop or loss of about 1.9 points on the I.Q. scale per generation. This again agrees very closely with Thomson's figure. (In my original memorandum I had myself attempted a calculation from the diagrams that he gave in his 1926 article, and got almost exactly the same figure. His I.Q. categories, however, were there expressed in terms of letters; in his paper he himself has since given fuller details of his classification, and finds a slightly higher figure of 2.06—practically the same. Cattell's inquiry *seemed* to suggest a more rapide decline, but I agree with Thomson's re-interpretation of his data.)

¹ The note since added by Professor Thomson on 'The Calculation of the Decline' seems to me now fully to meet any criticisms of the kind I had in mind when contributing to the discussion in January.

(ii) *The Increase of the Subnormal and the Decrease of the Supernormal.* In my own view, the important inference from this result is not so much that, owing to the differential birthrate, the general *average* of the population's intelligence is somewhat less than it would have been with an equal birthrate in all sections; rather, the serious feature seems to me the consequent changes in the *tail-ends* of the distribution. The decline in the general average is slight; but the effect of that decline is to produce an increasing proportion of dull and mentally defective children, and a diminishing proportion of children of high ability. The rate of decline, if maintained for 50 years, would roughly *double the number of feeble-minded* children, and halve the number of pupils of "scholarship" ability.

B. INDUCTIVE (DIRECT) EVIDENCE

In psychology, more than in any other science, it is highly precarious to trust solely to deductive arguments, without direct empirical confirmation. Our deductions tell us that during the past 25 years the number of certifiable defectives must have increased by at least one-third, and the number of supernormal children must have diminished by at least one-third.

(a) Again and again, particularly in the genetic field, mere deductions have not been confirmed. Indeed, even where we should expect the effects of war, etc., to produce large changes, the constants for the population have shown an unexpected stability. (b) Changes so large as those just computed would surely have attracted the attention of the older teachers, education officials, school medical officers, and the like. They *do* frequently complain of such changes in certain areas: when they do, the phenomena can nearly always be explained to a large degree by migration, changes in population and the like. (c) Repeated surveys of the same London areas do not reveal changes so large as this. However, even when the effects of migration, altered modes of ascertainment, etc., have been allowed for, they do indicate *some* decline. But the allowances are largely a matter of guess-work.

Practical Corollary. The real need is for further systematic research, especially a repetition of test-surveys in areas previously tested. Owing to the numerous pitfalls in such inquiries, the co-operation is needed of (i) psychologists, (ii) statisticians, (iii) geneticists, (iv) experts trained in social field-work.

PROFESSOR LIONEL PENROSE

ON reading Dr. Thomson's paper, I was impressed by the array of observed facts from which it was inferred that the mean level of national intelligence is likely to decline. Two essential assumptions, however, have to be made, if this inference is to follow correctly. Intelligence, as measured by tests, must be considered an inherited quality and differential fertility with respect to this quality, as Professor Carr-Saunders has emphasized, must not be considered a transient phenomenon. These assumptions set an awkward problem. Differential fertility is likely to have been significant during the last 50 or 100 years, but it could have been present much farther back still in history. Possibly it is a normal biological process. If so, there must be genetical or environmental mechanisms tending to keep the mean intelligence level constant. Otherwise, by now, there would be nothing but defectives left in the population. I have taken some trouble to search for influences, which would normally mitigate the effects of differential fertility with respect to intelligence, and it may be of interest to mention some findings.

Two points seem to stand out as possibly important. First, how far can the observed lower I.Q. of children in larger families be due to an actual falling off of intelligence in later births as compared with earlier ones? The suggestion is not unreasonable in view of the known effects of advancing maternal age in relation to foetal malformations and the known effects of repeated pregnancies in the formation of immune reactions in the mother unfavourable to the children in certain cases. Moreover, some direct observations have tended to show that, in pairs of sibs taken at random, the younger has a lower average I.Q. than the older (Herrman & Hogben, 1933). If this effect were large, it would seriously disrupt the arguments predicting intelligence decline. With universal family limitations, the mean I.Q. of the children would automatically rise. Secondly, although we are accustomed to regard low-grade deficiency as commonly brought about by the action of recessive genes, it does not seem to have been realized that deviations from the normal in the direction of very high intelligence may also be recessively determined. The tendency to inbreeding, shown in the pedigrees of some highly talented families, rather suggests that there may be recessive genes, which cause a condition of high intelligence coupled with low fertility. If, indeed, this were so,

people with average intelligence would be carriers of such genes and would form the main reservoir for replacement of the relatively infertile but mentally most capable part of the population.

Finally, knowing that physique and intelligence are positively correlated, I thought that a parallel experiment to those carried out by Thomson, Cattell, and others on I.Q. might be practicable with respect to weight or stature. In the city of Toronto, for example, 15,008 children were classified by Boas (1910) according to stature and size of sibship. These observations themselves were in no way invalidated by criticisms levelled at some conclusions, which Boas drew, when he compared immigrants with earlier settlers. A consistent trend for stature to be inversely correlated with size of sibship is demonstrable in these data at all ages for both sexes; the over-all correlation for constant age and sex for all children was 0.093 ± 0.008 . In more concrete terms, the mean stature of children with no sibs was almost one inch greater than the mean stature of children with about 10 sibs. Now, stature is believed to be genetically determined even more specifically than intelligence. Fisher (1925) estimated that only 5 per cent. of the variance of stature in sibs was due to non-hereditary causes. It seemed, therefore, quite fair to apply the technique used by Cattell (1937) for estimating rate of decline of mean I.Q. to estimate rate of decline of mean stature in Toronto children. Calculation showed that the mean stature could be expected to decline at the rate of rather less than a quarter of an inch per generation. However, in a report published by the Canadian Department of Trade and Commerce (1942) physical measurements of Toronto children taken in 1923 and 1939 were compared. It was shown that, during this brief half generation, the average child's stature had not diminished but had actually increased by an amount varying between one and two inches in different age-groups. This finding is not exceptional for it has been demonstrated (Karn, 1937) that the mean stature of English children has been steadily rising for at least 50 years. It does not, of course, follow from this that adult stature has increased in a similar manner. Nevertheless, the parallel of children's mean stature with children's mean I.Q. seems to me worthy of careful consideration and leads me to doubt whether pessimism about the trend of National intelligence is justified.

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PROFESSOR GODFREY THOMSON

I CAME to this meeting with no intention to take part in it, but to listen in the background, and it is only with some reluctance that I have yielded to Lord Horder's invitation to sum up the discussion.

It is very important in this problem, as in all scientific work, to keep separate the actual observed facts and the deductions drawn from them. The observed fact is that the score made by a child in an intelligence test is negatively correlated with the number of his brothers and sisters. This may be due to the educational handicaps of a large family, or to a tendency for intelligent parents to have few children, or (as I think) to both these influences. Professor Penrose has pointed to another possible cause, if *later* children in a family are less intelligent. Dr. Fraser-Roberts (whose unavoidable absence we so much regret) has told me that he is going over his Bath data with this point in mind, and finding reason to think that the later children *are* less intelligent. On the other hand, Professor Thurstone of Chicago writes that he finds the opposite. It is not very easy to disentangle the influences of size of family and position in family. We have in Moray House many records of test scores of complete age-groups of children in English counties, and intend, when time and staffing allow, to search them for possible light on this aspect of the problem.

The most striking part of tonight's discussion is Professor Penrose's report of the observations on the height of Toronto children, where a generation ago there was a differential relationship of fertility and stature (though a good deal less pronounced than in our case of fertility and intelligence test score), in spite of which there has since actually been a very considerable increase in stature among the children. Two thoughts occur to me about this. There may have been much migration, and the present children may be racially different from the earlier children. (But increase in stature among children is said to be widespread.) Or, if this explanation be ruled out, the increase, due presumably to better nutrition, may merely mask the genetic decrease temporarily. We can hardly expect improved nutrition to win in the long run against persistent selection even if it scores a short-term striking success. Improved educational nutrition may similarly mask a decline in inborn intelligence, but only for a time. A race between education and selection would, I fear, end in ultimate

catastrophe. But education can put the catastrophe off. However, I welcome the relief afforded by these hopes, for I would dearly like to think that remediable social handicaps can explain our observations, and that social improvements can remove our fears.

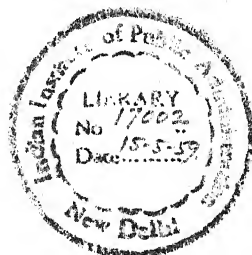
I am glad to be able to tell Sir Cyril Burt that the repetition of test-surveys in areas previously tested is already under way. In Scotland a complete age-group of 90,000 11-year-olds was tested in 1932, and this June, fifteen years later, the present complete age-group of 11-year-olds, now only 80,000 strong, will be tested. A sample of 1,000 was in 1932 given individual Binet tests and that will again be done. This time, moreover, a great deal of further information is to be obtained, about the kind of district around each school, the size of class, the place of birth, the position in the family, the existence of twins, etc. A random sample of 10 per cent. of the age-group will be visited in their homes and will provide still further information concerning evacuation during the war, the occupational status of the father, the age of the mother, the number of individuals in the home, the physical and medical condition of the child, etc. The guiding committee of the Scottish Council for Research in Education includes psychologists and statisticians of high rank, medical officers of health, directors of education, teachers, and others, and has the co-operation of very many more.

In England there exists no complete mental survey record of a complete age-group for the whole country. But in Moray House we have records of many complete age-groups of numerous towns and counties, and in response to our appeal about a dozen of these, whose combined age-group is 36,000 strong, are retesting this spring, using the same tests as were employed about ten years ago. From about half of them we hope also to get some of the above-mentioned additional information.

It is clear that these researches, interesting as they are, and much as it is our duty to carry them out, will not in themselves be conclusive. If the average intelligence test score has decreased, the critic can attribute that to the effect of the intervening war, though we may get differential evidence from districts much or little affected by the war. An increase may be attributed to greater familiarity with test-procedure, or to the greater prevalence of the regrettable practice of coaching children in intelligence tests. Even "no change" may be explained as a real decline marked by greater familiarity and by coaching! But it is our hope that careful statistical analysis of the mass of data may make some conclusions more probable, on better evidence, than is the case today.

Two English counties have already made their returns, one show-

ing a decline, the other no change. But a curious complication is showing itself of which we have further and more indirect evidence. On tests which formerly had no sex-bias girls are now beating boys, it would seem. Has the war affected girls and boys differently? Or are we faced with the even more terrible prospect of declining male, but increasing female intelligence? Joking apart, it is clear from all these difficulties that the problem we are tackling is not easy. Especially will the separation of the inborn and the environmental causes be hard to make. But it is certainly our duty to try.



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1. EUGENICS IN PROSPECT AND RETROSPECT

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2. INTELLIGENCE AND FERTILITY

By Sir Cyril Burt, 1946

3. THE TREND OF NATIONAL INTELLIGENCE

By Godfrey Thomson, 1947

4. PSYCHOLOGICAL APPROACHES TO THE BIOGRAPHY OF GENIUS

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